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Ridgway on the Anatomy of the Humming-Birds and Swifts.—A Rejoinder.—What I say here has reference to Mr. Ridgway's reply to my review of his "popular monograph of the Humming-Birds," which review appeared in the October *NATURALIST*, and his reply in the December following issue of that journal (1892, p. 1040).

In that reply Mr. Ridgway remarks that his description of the humming-bird's tongue "is substantially a condensation of MacGillivray's"³ (in Audubon's *Birds of America*, Vol. iv, pp. 197, 198), and that his "knowledge of the subject is based chiefly upon it." Now one of the errors I pointed out in Mr. Ridgway's "Humming-Birds" was that he claimed the tongue in those birds was "hollow," and yet he now states that his account is a condensation of Macgillivray's lucid description. Let me contrast the statement of the two authors, thus:

MACGILLIVRAY.

In the tongue of the humming-bird "there are, it is true, two cylindrical tubes, but they gradually taper away toward the points, and instead of being pervious, form two sheaths for the two terminal parts or shafts of the glosso-hyal portion of the tongue, which run nearly to the tip." (Aud. *Birds of Amer.*, Vol. iv, p. 198).

RIDGWAY.

In the humming-birds "The tongue is slender and very extensile, like that of the woodpeckers. * * * * * Instead, however, of its being as in the woodpeckers, solid and tipped with a barbed, horny point, it is hollow." (*The Humming-Birds*, p. 290).

These sentences, *in either case*, are completed by their respective authors in their calling attention to the bifurcated condition of the extremity of the tongue, and, as that is not the point in question, I purposely omitted it.

Farther on in his reply Mr. Ridgway remarks that his knowledge of the structure of the humming-birds is to some extent based upon the "later dissections of thirteen species," made by Mr. F. A. Lucas, "instead of one, as in the case of Dr. Shufeldt's 'extensive dissections.'" If my friend will again allow me to invite his attention to the literature of the subject, I would call it to my memoir published in

³If Mr. Ridgway will permit one who has long been familiar with the works of "the Scotch anatomist," I would "kindly invite" his attention to the fact that that careful dissector of birds spelled his name Macgillivray, and *not* MacGillivray, as Mr. Ridgway always writes it.—R. W. S.

the *Linnean Society's Journal*, Vol. xx (London), pp. 303, 304, where he will find that I had the following material for my "extensive dissections," namely, spirit specimens of *Cæligena clemencie*, *Trochilus colubris*, *T. alexandri*, *T. anna*, *T. platycercus*, *T. rufus*, *T. calliope* and *Iache latirostris*—in all 3 genera, 8 species, and 62 specimens. When this material was presented by me to the collections of the U. S. National Museum, Mr. Lucas remarked that it was the largest and best collection of spirit specimens of North American humming-birds ever donated to the institution by one individual. It naturally seems to me that the further Mr. Ridgway goes into this subject the greater is the display of his ignorance of the literature of it. Now when I charge a writer with being ignorant of the literature of a subject I take the pains, as I have done above, to point out wherein his ignorance lies, and do not resort to the remarkable method adopted by Mr. Ridgway—thus, and alluding in his reply to myself, he continues: "Apparently he is not familiar with the literature of the subject, for, if he were posted, he would know that leading authorities on avian comparative anatomy are overwhelmingly if not unanimously against his side of the question." Rather remarkable logic—especially to be immediately followed up in the very next sentence by, "I would therefore suggest that he consult Fürbringer, Parker, Garrod and Gadow, and thus learn regarding the matter which he handles with so much assurance. Even a careful perusal of Huxley (whom, by some strange hallucination, he imagines his abettor), may also prove instructive to him (p. 1040)." To this I would simply reply that in so far as Professor Fürbringer is concerned, I can say that, thanks to his generosity, I am the fortunate possessor, not only of many of his works upon comparative avian anatomy, but also his two massive and superb volumes so familiar to us all, and while I can thank him for his numerous and courteous references to my memoirs in that great work, I must dissent from his views in the matter of the taxonomy, of the Cypseli and Trochili. Apparently Professor Fürbringer had not seen my Linnean memoir, which was defended for me by Professor Parker before the London Linnean Society a number of years ago. There is a reason for that, however, as both works appeared early in 1888.⁴

⁴Shufeldt, R. W., Studies of the Macrochires, morphological and otherwise, with the view of indicating their relationships and defining their several positions in the System. Linn. Soc. Jour.—Zool., Vol. xx, pp. 299–394, Pls. xvii–xxiv (Communicated by W. K. Parker, F. R. S., F. L. S., 19th Jan., 1888). When I spoke of my extensive dissections this was the work I had reference to, and not to the brief note in *Forest and Stream*, as Mr. Ridgway seems to think. It stands to-day as the most extensive paper on the *entire structure* of the Humming-birds ever published by a single individual.

As for Professor Parker, I can say that I had the honor of being one of his constant correspondents for a number of years, and I have from him *several hundred* letters. A year or so before his lamented death it was his intention to supplement my work upon the "Macrochires," and I had sent him some material to that end. I have letters to show from him that he practically agreed with me in the taxonomy of the swifts, swallows and humming-birds, and he proposed to figure one of the latter "as big as a Cochinchina cock!" It is to the loss of the science of systematic ornithology that he never lived to accomplish it. Next as to Garrod. My library contains a copy of his "Collected Scientific Papers," a work I have had occasion to refer to almost daily since its appearance. He was disposed to classify birds upon altogether too few characters; there is no evidence whatever in his work that he ever critically compared the *entire structure* of a humming-bird with that of a swift; he notes simply the fact that "the tensor patagii brevis (in the Cypselidæ and Trochilidæ) and the pterylosis are characteristic, as is the sternum," but he failed to point out how widely they differed morphologically (p. 222). I dissent altogether from his views not only upon his taxonomy of the Cypseli and Trochili, but also from much else that he has left us upon the classification of birds. I must ask Mr. Ridgway for the title of any work from the pen of Professor Gadow wherein I may find a critical comparison of the *entire structure* of a swift and a humming-bird. I know of none, though I *do* know, as do many others interested in the classification of birds, that at the present time Dr. Gadow is hard at work upon probably what will prove one of the best *practical* schemes for the taxonomy of the class, and it will shortly appear. He has already done me the great honor in inviting me to submit my own views to him on the subject, and in a valuable letter I have just received from that able taxonomer I find that he is prepared to admit that the goatsuckers no longer should be retained in the same group with the forms they have usually been associated with heretofore, and he says, "I shall reconsider the position of the Caprimulgidæ and elevate them perhaps on account of Steatornis as Caprimulgi, thus making the difference from Cypselidæ and Trochilidæ more marked, although I shall not go so far as Fürbringer has done." Mr. William Brewster, of Cambridge, the distinguished American ornithologist, and one of the Committee responsible for the "Check-List" of the American Ornithologist's Union, and the classification therein set forth, having read my contribution to *The Ibis* (January, 1893) on Swifts and Humming-birds, writes me under date of February 13, 1893: "It

is comforting to find that there are now, among scientific men of distinction, champions of the belief of my boyhood days that a swift is merely a peculiar kind of swallow."

Now as to my "strange hallucination" in the matter of Professor Huxley's views upon the classification of the Swifts, Swallows, and Humming-birds. I have a well-worn copy of his famous P. Z. S. memoir of 1867—one of the most remarkable contributions ever written upon the classification of birds—and, I may add, a *perfect monumental beacon*, warning comparative avian anatomists for all time in their attempts to classify birds, against trusting to any single set of characters. In his "Cypselomorphæ" Huxley placed only the Swifts, Humming-birds and Goatsuckers (pp. 468, 469). It was done upon only too few characters, and mainly based upon the osteological ones seen at the base of the skull. But Huxley believed the vomer of a Humming-bird was "truncated at the anterior end," an error which both Parker and myself independently pointed out for him. But a quarter of a century is a long time in comparative anatomy (1867-1892), and if Professor Huxley has kept up with the literature of the subject he may hold entirely different views at the present writing. This would appear to be the more probable, as his misgivings were sufficiently strong as to have him write in his 1867 memoir that "In their cranial characters the Swifts are far more closely allied with the Swallows than with any of the Desmognathous birds, the Swift presenting but a very slight modification of the true Passerine type exhibited by the Swallow (*loc. cit.* p. 456). May I ask Mr. Ridgway what he thinks Professor Huxley meant when he wrote that sentence? And does he believe that to-day Professor Huxley would retain the Caprimulgi with the Humming-birds and Swifts together in one group and consider it to be a natural one?

One word more and I have done for the present. In the case of doubtful affinity among birds, as with all other forms, there is but one true way of getting at a solution, and that is to critically weigh and compare *everything* that is known about the one group with *everything* that is known about the other, before finally deciding. Now I challenge Mr. Ridgway to take any species of North American Humming-bird and any species of North American Swift, and arrange in two columns in a comparative way all that is known about either species, paleontologically, biologically, morphologically, or otherwise, and then upon summing up give any real reason why the Trochili and the Cypseli should be considered to constitute by themselves a natural group of birds. I have very serious doubts as to his ability to do this. If he

ever undertake such a task, he must bear one thing well in mind, and that he must not do it with the view of "exploding" any one's "peculiar notions," but rather with the view of getting at the truth of the matter. Then, too, do not condemn another's views simply because "leading authorities on avian anatomy are overwhelmingly if not unanimously against his side of the question (p. 1040)." That has been the case with a great many anatomists in the world's history, and even I can remember when the "leading authorities" overwhelmingly if not unanimously believed that *Chamaea* was a *Wren*; but the present writer in his published account⁵ based upon his "extensive dissections" claimed it was most nearly related to the Bush-Tits (of the genus *Psaltriparus*), to which opinion the "leading authorities," who have reviewed my work, are now rapidly coming. But no doubt Mr. Ridgway remembers something of this; he most assuredly does if he is familiar with the literature we have upon the subject.

R. W. SHUFELDT.

Animal Coloration.⁶—This volume has grown out of materials collected by Mr. Beddard for the Davis lectures delivered at the Zoological Garden of London in 1890. It contains the latest information as to the phenomena of coloration exhibited by animals, together with the most important theories upon the subject, including those of Dr. Eisig and M. Stolzmann, which had not previously found their way into works of a popular character. Mr. Beddard groups his facts under the following heads: Coloration Affected by Environment; Protective Coloration; Warning Coloration; Protective Mimicry; Sexual Coloration.

In summarizing the facts concerning Protective Mimicry, the author concludes that the theory of Natural Selection alone does not satisfactorily explain the remarkable resemblances included under that caption.

The chapter on Sexual Coloration calls attention to some objections to Darwin's theory of sexual selection, and gives briefly a sketch of the views of Mr. Stolzmann and those of Mr. Wallace.

Mr. Beddard's objections to the use to which the theory of Natural Selection has been put by various naturalists, and especially by Messrs

⁵ "On the Position of *Chamaea* in the System." The Jour. of Morph., Vol. iii, No. 3, pp. 475-502, figs. 1-8.

⁶ Animal Coloration, an Account of the Principal Facts and Theories Relating to the Colors and Markings of Animals, by Frank E. Beddard, M. A. Oxon., F. R. S. E., with four colored plates and wood cuts in the text. London, Swan, Sonnenschein & Co.; New York, Macmillan & Co., 1892.